

## Giving Cell Biology an Institutional Identity

In previous chapters I have focused on how new research techniques, especially electron microscopy and cell fractionation, made it possible for researchers to investigate mechanisms within cells. I have analyzed the development of the first products of these investigations – mechanisms for oxidative metabolism in the mitochondria, for protein synthesis in the endoplasmic reticulum, for protein transport in the Golgi apparatus, and for breakdown and disposal of cellular material in the lysosome. These endeavors were the focus of a new field of science that by the 1960s called itself *cell biology*. Many researchers chose this term intentionally to mark a distinction between classical cytology, concerned primarily with morphological structure, and the new, initially interdisciplinary enterprise that took on the challenge of integrating structural and functional information about the cell. By the end of the 1960s, this new scientific field had successfully occupied the terra incognita between cytology and biochemistry I identified in Chapter 3.

To become more than a temporary enterprise, cell biology needed its own institutional identity. Journals and professional societies are among the defining institutions of a scientific discipline. These provide important channels for scientists not only to disseminate their work, but also to receive credit for it. Publishing in a journal or appearing on a scientific program provides stature to scientists and evidence that their work is recognized by peers. Such institutions, though, play more than a certifying role. They also serve to direct inquiry by demarcating problems and methods for solving them that are accepted and valued by a particular community of scientists.

When a field begins by drawing upon practitioners of a number of existing disciplines, the journals and professional societies play an additional role – they serve to provide a distinctive identity for the new endeavor, enabling it to take its place among existing fields of science. Although many founding

members of the field of cell biology continued to identify themselves with the institutions of the field in which they were trained, as the new institutions took root, younger researchers tended to identify themselves as cell biologists.

Scientific institutions do not create themselves. Establishing and sustaining them requires effort, and those scientists who play leading roles in such institutions often devote a significant amount of their professional energies to them. Creating and maintaining institutions also involves deciding on the character of the institutions, and this in turn will shape the area of scientific research. In particular, these decisions will determine what kinds of research will be considered as part of that area of scientific research. In cell biology, as we have already seen in Chapter 6, two different styles of research were developed by Palade, Porter, and others at the Rockefeller Institute in New York and by Sjöstrand at the Karolinska Institute in Stockholm. Researchers within the broader spheres of influence of each institute sought to shape the contours of the emerging field not only by performing, presenting, and publishing groundbreaking research but also by helping to establish new journals and societies.

In Europe, the International Society for Cell Biology was established in 1947 under the umbrella of the International Union of Biological Sciences. (Its predecessor, the International Society for Experimental Cytology, had been founded in 1933.) In the same year the Institute for Cell Research was established at the Karolinska Institute with Torbjörn Caspersson as its director. In 1950 he, together with John Runnström, created the journal *Experimental Cell Research* and two years later Geoffrey Bourne and James Danielli created the *International Review of Cytology* (both sponsored by the International Society for Cell Biology).<sup>1</sup> In 1957 Fritiof Sjöstrand established the *Journal of Ultrastructure Research*.

In the United States, two institutions promoting cell biology as a new interdisciplinary field arose in the 1950s and 1960s. The *Journal of Biophysical and Biochemical Cytology* was first conceptualized in 1953 and already in 1955 had published its first issue; it switched to its current name, the *Journal of Cell Biology*, in 1962. The American Society for Cell Biology began in 1960, sponsored its first meeting in 1961, and formed an affiliation with the

<sup>1</sup> Between 1962 and 1970 the International Society for Cell Biology also sponsored a series of symposia. In 1972 the society dissolved itself in favor of the International Federation for Cell Biology, comprised of the American Society for Cell Biology, the European Cell Biology Organization (a federation of national and regional societies in Europe), and the Japanese Society for Cell Biology. The Federation held its first meeting in St. Louis in 1972, hosted by the American Society for Cell Biology.

*Journal of Cell Biology* in 1964. In this final chapter, I will explore in some depth the decision making that shaped these institutions and, through them, the field of cell biology in the United States and elsewhere.<sup>2</sup>

1. CREATION OF THE *JOURNAL OF BIOPHYSICAL AND  
BIOCHEMICAL CYTOLOGY*

The advent of electron microscopy was clearly one impetus for the creation of the *Journal of Biophysical and Biochemical Cytology*. The high resolution of the micrographs produced by the early 1950s required the capacity, which only a few journals possessed, to produce very high quality plates. In particular the *Anatomical Record*, one of the major journals interested in morphological issues, lacked the ability to produce sufficiently good plates.<sup>3</sup> Pioneers in employing electron microscopy for cell structures were therefore very interested in such a journal. Those journals that could produce the quality plates were often not interested in publishing numerous electron micrographs. This was borne out both in the event that most directly precipitated the creation of the new journal and in the subsequent correspondence. Porter, whose research I have discussed extensively in previous chapters, wrote a paper with Don Fawcett (Fawcett & Porter, 1953) identifying the 9 + 2 structure of cilia, which was rejected by the *Journal of Experimental Medicine* (published at Rockefeller).<sup>4</sup> Porter attributed the idea of starting a new journal to Herbert

<sup>2</sup> In part my decision to focus on these two institutions was guided by the fact that detailed records were available through the Rockefeller Archive Center and the American Society for Cell Biology's national office. I am grateful to both of these institutions and their staffs for generous and skilled assistance.

<sup>3</sup> In December 1953 Stanley Bennett, chair of the anatomy department at the University of Washington, wrote letters to the editors of both the *Anatomical Record* and the *American Journal of Anatomy* (both published by the Wistar Institute) complaining about the poor quality of half-tone engravings appearing in those journals.

<sup>4</sup> Porter commented on this episode in a short account he wrote of the birth of the *Journal of Biophysical and Biochemical Cytology* on 10 October 1959: "Many of the early papers were published in the *Journal of Experimental Medicine* – edited largely by Peyton Rous, who had the good sense to appreciate the significance of what was coming. The other editors of the J.E.M. and most especially Rene Dubos did not feel that the character of the J.E.M. should be changed to accommodate (sic) a lot of reports on morphological studies. A real storm developed around the one on cilia (Fawcett-Porter, 1954) submitted first to the J.E.M. in the early summer of 1953. Rous was away at the time, so Dubos had the final say – and the paper was refused. I recall that Vincent Dole in giving me the manuscript said he thought it an interesting and important paper but not for the J.E.M. He went on to say he thought the few of us interested in this new area should organize a new journal. Later that summer the same paper was submitted to the J.G.P. [*Journal of General Physiology*, another Rockefeller journal] and again rejected as unsuitable in content. I later, in September, discussed the matter with Dr. Gasser who had then retired and he

S. Gasser, who had just stepped down as director of the Rockefeller Institute. Porter took the idea to Detlev Bronk, who had replaced Gasser and was given the title of President of the Rockefeller Institute. Because the Institute already published two related journals, the *Journal of General Physiology* and the *Journal of Experimental Medicine*, it was a natural candidate to publish such a journal.<sup>5</sup>

Bronk was extremely receptive to the idea and, after discussing it with others in the emerging field (especially H. Stanley Bennett and Francis O. Schmitt), made a proposal that the Board of Trustees of the Rockefeller Institute approved in January 1954 to establish a *Journal of Cytology*. Bronk's proposal characterized the new journal as follows:

It is proposed that it deal with cytology from the point of view of the anatomist and histologist; the physiologist; the biochemist; and especially the histochemist, and those concerned with the development of the exciting new physical and chemical methods for investigation of the submicroscopic molecular structure of cells. This journal would deal with the results of studies made possible by the electron microscope, Xray diffraction methods, etcetera, but it certainly would not concentrate on such methods inasmuch as the course of cytology will ultimately move on with the aid of other unanticipated, undeveloped methods.<sup>6</sup>

From the outset, the focus of the journal was a point of contention. Porter's primary interest was electron microscopy and so a major objective for him was creating a journal able to reproduce electron micrographs with high enough quality. (Porter, for example, had several engravers prepare sample proofs so as to determine which would produce the highest quality plates.) A letter from Bennett to Bronk on 3 December 1953 emphasized a similar concern. After identifying the electron microscope as the principle instrument responsible for the recent advances in cytology, which put "the world . . . on the threshold of a classical period of advance in cytological knowledge, comparable in

was more than a little incensed because no one had asked his opinion. (He was listed as editor of both journals.) While in this mood he too said we should start a new journal" (folder 5, box 2, RU 518, Rockefeller University Archives, RAC).

<sup>5</sup> Although Porter committed himself to the creation of his new journal, he was not so kind when Sjöstrand proposed to create another one. In a letter to Franz Schrader on 8 June 1956 Porter stated, "I heard a few days ago that F. Sjöstrand, Stockholm, feels there is adequate excuse to start another journal for papers on fine structure etc. Academic would publish it. If you have any strong feelings against such a venture I wish you would make them known to Mr. Jacoby at Academic" (folder 10, box 1, RU 518, Rockefeller University Archives, RAC). Nonetheless, Sjöstrand's new journal, *The Journal of Ultrastructure Research*, began publication in 1957.

<sup>6</sup> Folder 10, box 1, RU 518, Rockefeller University Archives, RAC.

significance to the Vesalian advance in Anatomy, or to the classical development of microscopic anatomy in the nineteenth century,” he argued,

It is already evident that the publication outlets for this important field are inadequate. The requirements are that the publication journal be able to reproduce high resolution electron micrographs, or other illustrative material with a wealth of fine detail, and that the editorial policies of the journal lend themselves to publication of work of this kind. At present, the *Journal of Experimental Medicine* and the *Journal of Histochemistry* in this country and *Experimental Cell Research* and *Biochemica et Biophysica Acta* are preparing halftone engravings of the necessary quality, but the purposes of these journals are such that only a few cytological papers can be accommodated. The editorial practices of the *Anatomical Record* and the *American Journal of Anatomy* likewise lend themselves to the accommodation of some cytological papers of the sort under discussion, but the quality of the half-tone engravings in these journals is very poor, and suitable reproduction of high resolution micrographs is unsatisfactory.<sup>7</sup>

Schmitt (letter to Bronk of 9 February 1954), on the other hand, raised the concern that the new journal would be too focused on electron microscopy. He contended, “We now have journals which publish morphological work (J. Morph., Exp. Cell Res., J. C. C. P., J. Exp. Med., B.B.A., and many others); I take it we are not proposing to found essentially a ‘Journal of Electron Microscopy.’” He characterized the new journal as “devoted to biology at the molecular (or near-molecular) level”:

Accordingly, what I should hope would be brought together in one journal would be micromorphology by direct methods (electron microscopy, UV, phase contrast, interference and other types of microscopes) and by indirect methods (X-ray diffraction, polarization optics) together with physical chemical investigations of the shapes, sizes and properties of particles (particularly macromolecules of biological interest) *in solution* (e.g., ultracentrifuge, electrophoresis, light scattering, streaming birefringence, viscosity and diffusion). Of course the aim of all of this is the detailed characterization of protoplasmic constituents.<sup>8</sup>

Schmitt was himself an important contributor to the development of electron microscopy, although primarily of viruses. His focus, however, was more broadly biophysical.

<sup>7</sup> Folder 10, box 1, RU 518, Rockefeller University Archives, RAC.

<sup>8</sup> Folder 10, box 1, RU 518, Rockefeller University Archives, RAC.

In part, this debate played itself out in setting up the board of editors. Porter (letter to Bronk, 2 February 1954) initially put forward a list of eleven names: Bennett, J. Norman Davidson, Edward Dempsey, Berwind P. Kaufmann, Lehninger, Daniel Mazia, Palade, Porter, Schmitt, Arnold Seligman, and Albert Frey-Wyssling. Schmitt (letter to Bronk, 9 February 1954) objected that Porter's list overemphasized descriptive morphology; he identified Bennett, Dempsey, Palade, and Porter as all representing this area. Schmitt proposed several alternative candidates. Bronk's final choice of eight editors included six from Porter's initial list of eleven. Three were pioneers in electron microscopy who were moving beyond traditional cytology to establish cell biology as a new discipline: Bennett (University of Washington), Palade (Rockefeller), and Porter (Rockefeller). The others were Lehninger (biochemist, Johns Hopkins), Schmitt (biophysicist, MIT), and Seligman (histochemist, Sinai Hospital of Baltimore) from Porter's list; Richard Bear (crystallographer, MIT) from Schmitt's suggestions; and Franz Schrader (cytogeneticist, Columbia) as Bronk's own addition. Because the editorial offices were located at Rockefeller, Porter and Palade were the *resident editors* overseeing the ongoing operations of the journal. Palade managed the incoming manuscripts, sending each to two of the editors. If they agreed on publication or rejection, that was the final decision; if they disagreed, a third editor or a specially chosen referee decided the question.

The question of the focus of the journal, however, went beyond deciding on editors and procedures for evaluating manuscripts. When the editors met with Bronk on 12 April 1954, they debated the name of the journal. Some objected to *Journal of Cytology*, the working name for the new journal, on the grounds that *cytology* conveyed too much of a focus on morphology and not enough on the functional perspectives. Accordingly, the adjectives *biophysical* and *biochemical* were added to the title.<sup>9</sup> The editors also arrived at a mission statement for the new journal that reveals significant concessions to Schmitt's concerns:

It will be the function of this new publication to provide a common medium for the presentation of morphological, biochemical and biophysical studies of the structure of cells and their components and of the functions of these components. The Journal will give special attention to investigations dealing with cellular organization at colloidal and molecular levels. Papers will be favored

<sup>9</sup> According to George Pappas (Interview, 23 October 1995, University of Illinois Chicago), Francis Schmitt and Paul Weiss were principally responsible for the new name. Hewson Swift (Interview, 24 October 1995, University of Chicago) maintained that the title was likely Porter's. This seems less likely, given Porter's later enthusiasm for changing the name.

which integrate information derived from newer approaches to cytology, such as histochemistry, cytogenetics, cytochemistry, electron microscopy and X-ray diffraction. Because of the significance of photographic evidence, the publishers of the Journal will endeavor to insure excellence of photoengravings and printing.

The first issue of the *Journal of Biophysical and Biochemical Cytology* appeared in January 1955. Including the words *biochemical* and *biophysical* in the title did not guarantee that these would figure centrally in the journal, and Schmitt's concern that the new journal not become a journal of electron microscopy continued to occupy the editors at their annual meetings during the journal's initial years. For example, in reporting to Bronk on the April 15, 1956, meeting of the editors (a little over a year into publication), Porter related, "It was agreed that morphological studies had been well represented among the published papers but that papers more properly defined as biophysical and biochemical in nature had been in short supply. Drs. Bear and Lehninger agreed to give this problem their personal attention." But the problem continued in subsequent years.<sup>10</sup> In their 1959 report to the other editors, Porter and Palade, the resident editors, related,

The editors have all expressed some concern about the predominance of reports on cell and tissue fine structure, derived from electron microscopy. We have therefore made an analysis of trends in the subject matter of manuscripts contributed and are pleased to report that it shows the Journal receiving a steadily increasing number of papers of the non-E.M. variety. Early in volume 4, e.g., reports primarily dependent on electron micrographic evidence exceeded the others by 2 to 1 whereas in the last number of volume 4 they were equal. Since then this trend has continued and, of the manuscripts currently available to us, only 2/5 report solely E.M. evidence. It would seem that our contributors and readers now recognize that the Journal is interested in cell biology regardless of the experiment, device or technique used to acquire the information.<sup>11</sup>

<sup>10</sup> Schmitt, responding to a summary of the 1957 editors' meeting that he had to miss, wrote to Keith Porter on 18 May 1957: "I shall personally try to secure more biophysical and biochemical papers, for I agree that descriptive morphological papers far outweigh the more physical and chemical ones" (folder 11, box 1, RU 518, Rockefeller University Archives, RAC). Even though Porter himself identified closely with electron microscopy, he too recognized the need for a broader range of papers. In a letter welcoming Donald Fawcett as a new editor of the journal (9 October 1959) Porter commented, "Anything you can do to keep your friends from sending us long descriptive manuscripts on anything and everything they can get into the E.M. will be appreciated. More "biophysics" is what we need!" (folder 11, box 1, RU 518, Rockefeller University Archives, RAC).

<sup>11</sup> Folder 11, Box 1, RU 518, Rockefeller University Archives, RAC.

Despite this optimistic assessment of success in moving beyond electron microscopy, though, the topic of diversity of manuscripts continued in subsequent years.

One of the principal ways in which the journal sought to attract manuscripts in areas other than morphology was to have particular editors represent these domains. Thus, when the question of adding or replacing editors arose, one of the primary considerations was securing scientists with appropriate specialties to attract manuscripts. For example, in 1958 the editors voted to expand their number to ten. Although they advanced four names, Porter (letter to Bronk, 29 August 1958) made a special comment on two of them, Paul Doty and Daniel Mazia: “Doty was selected because he is a biophysically oriented investigator. Mazia – because he is a cell physiologist.” At the urging of Bronk, the editors began to replace themselves in 1959 (drawing lots for the “privilege” of retiring). The editors recommended replacements from the same research area as the retiring member so as to continue to cover a broad range of research areas. Accordingly, when Schmitt and Porter were chosen to retire, and Bennett resigned, Bernard Davis was selected to replace Schmitt and Don Fawcett to replace Bennett. (A majority of editors wrote in Porter’s name, arguing that because he was *de facto* the chief editor, he was needed to provide continuity.)

Another strategy the journal tried was the creation of special supplemental issues. The first of these, which appeared as a supplement to volume 2 in September 1956, consisted of papers presented at a Conference on Tissue Fine Structure in January of that year. At the first meeting of the editorial board on April 15, 1956, the members had discussed occasionally reprinting classical papers translated into English. They revisited the suggestion at several meetings and finally agreed to reprint a classic paper along with contemporary papers by major researchers on the same topic. The editors sought external funding for this effort, and received a grant for \$15,000 from the United States Public Health Service. Porter edited the first such supplement (appearing in August 1961); it focused on muscle and was organized around a translation of Emilio Veratti’s 1901 paper “Investigations on the Fine Structure of Striated Muscle Fiber.” Papers by C. R. Skoglund; James F. Reger; David S. Smith; Don W. Fawcett and Jean Paul Revel; Lee D. Peachey; Alexander Mauro and W. Robert Adams; U. Muscatello, Ebba Andersson-Cedergren, G. F. Azzone, and Alexandra von der Decken; and Keith Porter accompanied it. Daniel Mazia edited a second special issue on mitosis, built around Walther Flemming’s 1880 “Beiträge zur Kenntnis der Zelle und Ihrer Lebenserscheinungen,” which appeared in April 1965.



In one sense, the attempt to incorporate biophysics and biochemistry into the journal ended in failure. In a 1960 report as resident editors to the other editors (30 April 1960), Porter and Palade, in the context of looking toward the future, raised the question of dropping *biophysical* and *biochemical* from the name of the journal:

First, again, is the problem of scope . . . As part of this it might be good to consider changing the name of the Journal, so that it more adequately describes the content, as it is now and will probably be. When the present title was decided upon, there was no *Journal of Biophysics*, and no *Journal of Molecular Biology*, and no weekly, publishing *Biochemical and Biophysical Research Communications*. As long as it makes space for them, the *J.B.B.C.* will continue to get a few manuscripts that might just as readily be published in one of these others or in *Biochemica [et] Biophysica Acta*.<sup>12</sup>

(Because that summer Palade wrote a letter to Porter opposing the name change<sup>13</sup> and cast the only vote against changing the name, it is likely that this statement represented only Porter's view.) At the 1961 meeting of the editors the name *Journal of Cell Biology* was proposed because it "better defines the purpose and scope of the Journal and is less cumbersome than the present one."

Porter formally canvassed the editors on the question of a name change in a letter of 23 June 1961 and put forward his own arguments for changing the name. He noted that the original name was a compromise, and not one that everyone endorsed: "The Journal was given its present name at the insistence of a few of the founding editors." It was chosen "to discourage a predicted flood of papers on pure morphology." While acknowledging that the choice of name may have brought a few papers properly characterized as "biophysical," Porter commented, in a pointed dig at Francis Schmitt, "Among these, it may be added, there was never one from the editor who insisted with greatest vehemence that 'biophysical' be included."<sup>14</sup> He also identified as a factor accounting for the paucity of submissions that were biophysical or

<sup>12</sup> Folder 3, Box 1, RU 518, Rockefeller University Archives, RAC.

<sup>13</sup> In a letter to Porter on 27 June 1961, Palade offered two reasons for keeping the name – the journal had already obtained a positive reputation with the name and the adjectives *biophysical* and *biochemical* characterize the trend in the field. He contended that the name *cell biology* represented "to a certain extent, a withdrawal to a more conservative position centered on cell structure and cell physiology in the traditional sense." He then proposed yet another alternative: *Cellular and Subcellular Biology*. (Folder 11, Box 1, RU 518, Rockefeller University Archives, RAC.)

<sup>14</sup> Schmitt did publish one paper in the journal, a short note presenting electron micrographs of ultracentrifuge preparation of paramyosin (Locker & Schmitt, 1957).

biochemical that other journals were now occupying those niches, including Rockefeller's own *Biophysical Journal*. De facto, therefore, the content of the journal was concentrated in areas that he viewed as properly known as *cell biology*. Porter indicated that the name change would thus reflect the niche that the journal inhabited, partly as a result of pressure from other sources. He suggested that such external determination of the niche may not be a bad thing: "If we acknowledge this and keep the initiative we may effectively serve the increasing interest being shown in the cell as a unit and in the correlation of fine structure and function." The new name was approved later that summer and the first issue as the *Journal of Cell Biology* (volume 12, number 1) was published in January 1962. With this change the journal and, indirectly, the domain for the discipline came to fit more closely Porter's own vision.

The newly-named journal was clearly very successful. It expanded rapidly in terms of subscribers, number of submissions, and size and rate of publication. Initially the journal had appeared every other month with a volume consisting of the year's six issues. In 1959 the number of pages per issue was increased and a volume was redefined as including only three issues (thereby allowing the journal to increase the annual price without increasing the price per volume). Yet the number of submissions continued to increase so rapidly that as of April 1960 Porter reported a backlog of over 100 accepted manuscripts. Accordingly, an additional issue was published in July 1960 as a temporary stopgap, and publication expanded to monthly in 1961. The journal had reached 1700 subscriptions by January 1960 and 3000 by 1965.

Content became more diverse as the number of pages and subscribers multiplied. The journal had already been moving beyond morphological studies of cells with electron microscopy before its change of name, and less than two years later Executive Editor Raymond Griffiths<sup>15</sup> was able to announce in The Annual Report for 1963, "Papers on cell fine structure alone no longer predominate; a steadily increasing percentage of submitted manuscripts pertains to the investigation of other aspects of the cell and its products, and to studies utilizing various combinations of technical approaches – of electron microscopy, cytochemistry, biochemistry (cell fractionation, isolation and analysis of cell components), biophysics (autoradiography), molecular biology, as well as others – in the analysis of basic problems of the cell. Especially noticeable is the trend toward more reports focusing on the quantitative aspects of biological events."<sup>16</sup>

<sup>15</sup> Griffiths was hired as Executive Editor of the Journal in December 1960. He held a Ph.D. from Princeton and an M.D. from Northwestern and had previously managed and edited publications of the American Cancer Society.

<sup>16</sup> Folder 4, box 1, RU 518, Rockefeller University Archives, RAC.

Porter's 23 June 1961 letter arguing for the new name had brought up one final consideration: the emergence of The American Society for Cell Biology (ASCB). Porter portrayed the society as a likely usurper unless a change of name brought alliance instead: "Should we decide to make this revision in name, we must move quickly. The immediate danger is that someone else will start a journal with these specific aims and this suggested name. One new organization – The American Society for Cell Biology – is particularly menacing in this regard. If, in fact, the editors of the *J.B.B.C.* recommend the change, the new society will be encouraged more than now to adopt the Journal as its official publication; and there is much to be said for society-sponsored journals, especially in guaranteeing the publication a long life." The reference to a "menacing" threat from the American Society for Cell Biology is particularly interesting because Porter himself was playing a major, if not the major, role in shaping the society and others involved had been, to that point, reluctant to consider starting a journal. Porter achieved his goal of bringing the two institutions into alignment. Shortly after the journal's name was changed to the *Journal of Cell Biology*, the ASCB approached Bronk with the suggestion of becoming a cosponsor of the journal. I will return to the question of society sponsorship after discussing the origins of the ASCB.

## 2. CREATION OF THE AMERICAN SOCIETY FOR CELL BIOLOGY

The initiative to create a society for cell biology in the United States appears to have originated with Paul Weiss, who already was active in the International Society for Cell Biology. He sought action toward this goal from the U.S. National Committee for the International Union of Biological Sciences (a committee under the auspices of the U.S. National Academy of Sciences, which is officially responsible for U.S. membership in the international unions for various scientific disciplines). They complied by passing a resolution on 6 April 1959 calling for the establishment of a "national society of cell biology to act as a national representative to the International Society for Cell Biology." This resolution was transmitted to Morgan Harris, president of the Tissue Culture Association (TCA), a technique-based society devoted to fostering use of tissue culture as a research tool within biology and medicine.<sup>17</sup>

<sup>17</sup> The TCA was created (initially with the name Tissue Culture Commission) in 1946 at the conclusion of a three-day conference in Hershey, PA, funded by the American Cancer Society and sponsored jointly by the panels on cellular physiology, cytochemistry, and nutrition of the Committee on Growth. The commission had two aims: to prepare and make available

Such a resolution fit well with the aspirations of Harris and other members of the TCA who sought to refocus the society around a subject matter rather than a technique.<sup>18</sup> The TCA made a proposal to the NIH Study Section for Cell Biology to fund a committee to “improve working relations among cell biologists.” The Study Section for Cell Biology, the primary vehicle in NIH for providing grants for research, was established in 1958 as a result of splitting what had been the Study Section for Morphology and Genetics into two sections, one for genetics and the other for cell biology (Copeland, 1999).<sup>19</sup> The Study Section unanimously approved the proposal and agreed to fund a meeting of up to ten cell biologists to consider how to promote this end. Harris was appointed chair of this committee, and he, in turn, selected Keith Porter

media for tissue culture at reasonable cost and to publish a bibliographic index of tissue culture methods (first published in 1952 under the title *Index of Tissue Culture*). It also ran a number of summer courses in Cooperstown, New York. Keith Porter was selected as chairman of the committee charged to establish the commission. In 1949 the name was changed to Tissue Culture Association (TCA) and George Gey was elected its first president. The TCA held annual meetings, often in conjunction with the American Association of Anatomists (see Copeland, 1999).

<sup>18</sup> In 1959 Morgan Harris, as President of the Tissue Culture Association, sent a Bulletin to members advancing the idea, developed by John Paul, Don Fawcett, and W. F. Scherer, of reorganizing the TCA to focus on cell biology. He first noted that since tissue culture had come to be “a standard part of the experimental armamentarium,” the need to promote tissue culture as a technique had declined. He then put forward the idea of focusing on a subject matter and identified cell biology as comprising a central interest of the membership. Specifically, he proposed that “an appropriate reorganization of the present TCA might result in a timely and useful society of cell biologists – cytologists, physiologists, biochemists, morphogeneticists, etc.– including those not now in the field of tissue culture but whose interests concern the cell.” At the business meeting of the TCA in Atlantic City in April, 1959, forty-seven members voted to explore such reorganization but eleven were opposed, apparently some of them quite vociferously. After the initial meeting to form the ASCB, however, the TCA elected to remain independent of it and “let nature take its course.” In 1994 the society changed its name to the Society for In Vitro Biology.

<sup>19</sup> One of the Study Section’s first initiatives was a proposal for University Laboratories for Cell Biology. The report approved by the study section on 27 September 1958 identified the first draft as having been “prepared by an *ad hoc* group including Dr. Keith Porter, Chairman, Dr. Clifford Grobstein, Dr. Heinz Herrmann, Dr. Daniel Mazia, Dr. Ernst Scharer, Dr. Van Potter and Dr. Herbert Taylor.” Their idea for these laboratories was partly modeled on the various national institutes comprising the NIH, but was explicitly to be decentralized to “college campuses, where the scientific staff would be in contact with students.” This proposal was not well received by the NIH administration and the Study Section instead proposed to support long term funding of highly qualified groups of individuals in a manner less restricted than individual grants. The proposal was that the NIH would maintain such a program for at least twenty-five years. A version of this plan was implemented and some grants were given, including one to the University of California, Berkeley, for a group involving Daniel Mazia, Morgan Harris, Max Alfert, and others (Interview with Morgan Harris, 9 December 1995, University of California, Berkeley).

to serve as chair pro tempore for the meeting and identify the appropriate cell biologists. In developing his list of cell biologists, Porter emphasized the need to bring in physiologists and biochemists to ensure balance (letter of Porter to Harris, 21 October 1959).

One problem this initiative faced was that other existing societies had come to recognize the cell as a prime area of focus and began jockeying for control of the new field of cell biology. An example is the Society of General Physiologists (SGP), itself created as recently as 1946.<sup>20</sup> Kenneth Thiman had recently prepared a report recommending that the SGP expand to include cell biologists and geneticists. C. L. Prosser, who was asked to represent the SGP, wrote to Porter (22 December 1959):

As you well know, the Society of General Physiologists is moving rather rapidly in the direction of becoming a society for cell biology. We feel our symposia have satisfied a real need in this area, and with the organization of a new division of comparative physiology within the American Society of Zoologists, it appears to us that the real place for the cellular approach to biological function is in the Society of General Physiologists. There is a good indication that the Society will have a journal affiliation shortly.<sup>21</sup>

Thus, a key question for those interested in establishing a society for cell biology was how to position it with respect to existing societies.

Porter's committee met at the Rockefeller Institute on 9 January 1960. The meeting began with remarks from representatives of the different constituencies: Morgan Harris of the TCA; E. G. Butler as chair of the Cell Biology Study Section; Paul Weiss, representing the International Society for Cell Biology; William McElroy and Teru Hayashi, representing the Society of General Physiologists; and Montrose Moses, Teru Hayashi, and A. K. Solomon, as organizers of recent Gordon Conferences on cell structure and metabolism. McElroy and Hayashi reported that by then the Society of General Physiologists had rejected the proposal to become a society for cell biology and supported forming a new society for cell biology. Thirteen of the fifteen scientists present then voted to create a new society to be named the

<sup>20</sup> According to Morgan Harris, he approached Dan Mazia when Mazia was President of the Society of General Physiologists and proposed to merge the TCA and the Society of General Physiologists into a Society for Cell Biology but was rebuffed. Interview with Morgan Harris, 9 December 1995, Berkeley, California.

<sup>21</sup> Indeed, in September 1960 the *Journal of General Physiology*, which had been published by the Rockefeller Institute since 1918, became the official organ of the Society of General Physiologists.

American Society for Cell Biology. Two abstained, but it is not clear who they were. The committee decided that those voting in favor would form the provisional council for the new society, and yet fourteen of the fifteen attended the next meeting on 28 May 1960. The only person who did not attend was McElroy, the representative of the SGP, but even his name appeared on the list of the provisional council, which was supplied in the October 1960 proposal to the NIH to fund the first scientific meeting of the society as well as in the official announcement of the ASCB in November 1960. Keith Porter was selected to chair the provisional council and Morgan Harris was elected secretary.

As with the *Journal of Biophysical and Biochemical Cytology*, a major question for the new society was how to ensure the desired mix of specialties in order not to find itself limited to morphology. The proposal to the NIH to fund the first scientific meeting stated, "Membership is expected to include biochemists, biophysicists, cytologists, histologists, microbiologists, physiologists, and others having a common interest in the cell" and made it clear that the program for the first meeting was to "facilitate interdisciplinary communication among cell biologists." One of the prime vehicles for accomplishing this end was the inclusion of invited symposia at the annual meetings. For the first meeting, held in Chicago in November 1961, symposia were organized on three topics: cell continuity, cell diversification, and the characters of cell interfaces. The program committee, chaired by Hewson Swift, sought to include speakers from the broad list of disciplines identified in the NIH proposal.<sup>22</sup> The bulk of the program consisted of contributed papers, which were accepted without review. Albert von Szent-Györgyi presented a banquet address entitled "The fusion of biological dimensions." A sense of the

<sup>22</sup> To recruit attendees for the first meeting and members for the society, the ASCB used a mailing list consisting of biologists included on the membership lists of the American Society of Anatomists, American Society of Naturalists, American Society of Zoologists, Botanical Society of America, Genetics Society of American, Gordon Research Conference on Cell Structure and Metabolism, Histochemical Society, Radiation Research Society, Society for Cell Biology (presumably the international society), Society of Protozoologists, and Tissue Culture Association. It is noteworthy that biochemical and biophysical societies are absent from this list, although additional names were later added from the membership list of the American Society of Biological Chemists. In November 1960, 2808 printed announcements were sent to the list thus compiled and by 27 January 1961, a total of 1187 replies requesting applications and additional information had been received. In the minutes of the 28 January 1961 meeting of the provisional council, it was noted that "most of the fields related to cell biology, such as biochemistry, microbiology, anatomy, pathology, histochemistry, cytology, electron microscopy, tissue culture, etc., were well represented. Botany and biophysics were two areas that were not well represented."

research interests of those attracted to the meeting is provided by the sessions into which these papers were organized in the program:

First meeting of the ASCB, 1961

Symposia

*Cell Continuity*

Rollin Hotchkiss, Rockefeller Institute, Continuity at the molecular level

Hans Ris, University of Wisconsin, Continuity of cytoplasmic organelles

Tracy Sonneborn, Indiana, The genetic control of cytoplasmic organization

*Cell Diversification*

Arthur B. Pardee, Princeton University, Diversification of bacteria in different environments

Morgan Harris, University of California, The evolution of somatic cell populations in vitro

F. C. Steward, Cornell University, Totipotency and variation in cultured cells (Some metabolic and morphogenetic manifestations)

*Characteristics of Cell Interfaces*

George Palade, Rockefeller Institute, The membrane systems of the cytoplasm

H. Passow, University of Hamburg, Membrane structure and ion permeability in red cells

Peter Mitchell, University of Edinburgh, The chemical asymmetry of membrane transport processes

A. D. McLaren, University of California, Effect of pH on reactions at biological interfaces

*Contributed Paper Sessions*

DNA and related topics (3 sessions)

Cell fine structure (2 sessions)

Cell diversification (3 sessions)

Permeability and related topics

Cell particulates (2 sessions)

Cell-parasite interaction

RNA and related topics

Enzyme location

Mitosis

Chromosome structure; protein synthesis

Cell movement

Cell culture

The society meetings were, from the outset, extremely successful. The first meeting in Chicago was attended by 844 scientists, and 744 applied for membership. The society, however, faced some of the same problems as the journal

in achieving the desired interdisciplinary mix. Disciplinary mix was the topic that dominated discussions in both the council and the executive committee of the ASCB in its early years. One upshot of these discussions was the strategy of using the invited symposia to bring in more biochemists and biophysicists. After the first meeting, the ASCB also decided that accepting all submissions was not possible, so a limit was placed on the number of contributed paper slots for the 1963 meeting. It was also determined that, in addition to quality, decisions as to which papers to accept would be based in part on disciplinary approach: "It was pointed out that some control over the program content might be necessary in order to attract, for example, biochemists, who are needed by the Society, and to prevent the Society from becoming identified with any one technique or discipline (for example, fine structure)" (Minutes of ASCB Council Meeting, 4 November 1962).

The society followed much the same strategies as the journal in attempting to deal with this issue. One strategy was to include biochemists and more functionally oriented scientists on the council in hopes that they could help attract members. Thus, David Green, a biochemist and one of the codirectors of the Institute for Enzyme Research at the University of Wisconsin, was included in the second meeting of the provisional council and was elected as one of the members of the council at the first meeting of the society in 1961. (By this time, however, Green's credibility among biochemists was in some doubt. After his proposal of a cyclophorase system for oxidative phosphorylation, discussed in the previous chapter, his work was regarded as increasingly speculative.) At the same meeting Alex Novikoff, a biochemist and histochemist at Albert Einstein College of Medicine, became president-elect and accordingly served as the society's second president in 1962–3. In appointing the nominating committee in January 1963, Novikoff followed a recommendation of the council and requested that the nominating committee nominate two biochemists to run for president-elect. The committee complied by nominating Van Potter and Harry Eagle, and Potter won the election to become the fourth president of the society.<sup>23</sup>

For the 1964 meeting of the society David Green served as program chair, and the resulting program had a strong biochemical presence. The invited symposia became a three-part symposium on biological membranes. The first part focused on three topics: energized ion translocation in membrane systems, contractile proteins of membrane systems, and structural proteins

<sup>23</sup> Hewson Swift, whose research embraced both electron microscopy and cytochemistry and focused on processes in the cell nucleus, was elected vice president when Novikoff became president, and immediately succeeded Novikoff as the third president.



of membrane systems. The second part focused on elementary particles in membrane systems, and the third on synthesis of mitochondria. The overall topic, and especially the second symposium, was closely related to Green's own interest in particles that could be isolated from the mitochondrial membrane and figured in mechanisms of electron transport and oxidative phosphorylation. The invited portion of the program included not only a number of researchers from Green's home institution of the University of Wisconsin (especially the Enzyme Institute) but also Britton Chance and Elfrain Racker, two biochemists who often differed with Green on interpretation of the particles he had isolated from the mitochondrial membrane. The following year another biochemist, Lee Peachey, served as program chair and he followed the same strategy as Green, organizing a three-part symposium. Its topic was sub-cellular movement, with one part focusing on physical and physical-chemical aspects (electron and proton mobility, movement of functional groups, and molecular movement, including conformational changes and movement of molecules), another part on translocation of ions and small molecules (ionic movement in mitochondria and other membrane systems), and a third on recent progress on biological movement (chromosome movement, spindle fibers, flagella, cilia, etc.).

These efforts did provide a significant presence of biochemistry on the programs of the ASCB, and a few biochemists became affiliated with the ASCB over the long term (e.g., Novikoff, Rollin Hotchkiss, Philip Siekevitz). The broader goal, however, was not achieved. Biochemists, like biophysicists, already had their own institutions and journals, which provided their primary institutional base. As cell biology became more institutionalized and developed its own departments in universities as well as journals and societies, cell biologists incorporated those tools of biochemistry that were important to them in the practice of cell biology. Biochemists had already become the primary users of cell fractionation techniques, adopted from early cell biology. Accordingly, the concern with reaching out to biochemists declined over time.

One of the main issues that confronted the new society was the question of sponsoring a journal. It arose at the first meeting of the permanent ASCB council on 9 December 1961 (the provisional council had deferred all matters that might imply long-term commitments for the society). Philip Siekevitz advanced a proposal for affiliation with the *Journal of Cellular and Comparative Physiology*, published by the Wistar Institute of Anatomy and Biology. That journal, which had begun publication in 1932, was widely viewed as in need of reinvigoration. Hewson Swift and Alex Novikoff reported on discussions with James Danielli, editor of *International Review of Cytology*,

about how the society might help improve the quality of that journal. Later in the meeting the suggestion of affiliating with the *Journal of Biophysical and Biochemical Cytology* was raised. To encourage interest in this third option, Porter reported on the nature of the linkage between the *Journal of General Physiology* (also published by Rockefeller University Press) and the Society of General Physiologists. At the meeting a motion was passed to appoint a committee to study publication options, but apparently no action was taken immediately.

The next spring Runnström invited the ASCB to appoint one or more representatives to the editorial board of the international journal he edited, *Experimental Cell Research*. At this stage Fawcett asked Swift to chair a Committee on Publication (the other members were Novikoff, Porter, Prescott, Siekevitz, and Solomon). The committee moved quickly and by the annual meeting in November 1962 made its recommendation that the society associate with the newly renamed *Journal of Cell Biology*. Bronk accepted the proposal the next month and in the spring of 1963 the ASCB membership approved it. A procedure was established so that, starting in 1964, the ASCB began to nominate half of the editors for the journal (the society nominated two candidates for each society slot on the editorial board, and the current editors selected among them). As part of its commitment to involve biochemists more centrally, the society emphasized biochemists among its nominees.

The activities of a professional society such as ASCB extend beyond sponsoring annual conferences; another important function is to promote knowledge of cell biology in the broader community. With the third meeting, held in New York City in November 1963, the society began to sponsor a pre-meeting for high school teachers. The first pre-meeting was held at the Rockefeller Institute and was co-sponsored by the High School Biology Teacher's Association.<sup>24</sup> Organized by Philip Siekevitz, the meeting featured presentations by George Palade, Van Potter, Wolfgang Joklik, and David Prescott. Starting with the seventh meeting in 1967, the society sponsored a symposium for high school students that involved both a number of talks and a guided tour of the commercial exhibits designed to educate students as to the kinds of instruments used in research (not to mention making the public more generally aware of their cost).<sup>25</sup>

<sup>24</sup> This was the name stated in the minutes but presumably the cosponsor was the National Association of Biology Teachers.

<sup>25</sup> The sessions for high school students appear to have been very successful. Joseph Gall urged society members not to attend the session because a very large turnout was expected and there might not be enough space. A second symposium for high school students was held in 1968 with talks by R. Perry, H. Lyman, and D. Beattie.

The idea of a brochure outlining opportunities for careers in cell biology was first put forward in the context of this symposium. Birgit Satir took on the responsibility for writing “Cell Biology: A Guide to Opportunities.” The guide provided a brief introduction to the history of and current opportunities for new discoveries in cell biology, as well as information about training and job opportunities (including likely salaries) in cell biology. It was published in 1969 and, in a letter to the membership on 25 June 1969, ASCB Secretary George Pappas reported that they were filling orders rapidly and “undoubtedly there will have to be another printing.”

The young society clearly struck a chord with a significant number of researchers, including many just beginning their careers. Winston Anderson, for example, commented, “Consider the national environment during this period – the ASCB was only 2 years old, and the founders, including Fawcett, Palade, Porter, and Swift, ruled supreme. We were all caught up in this national enthusiasm and as cell biologists considered ourselves ‘cytonauts’ – exploring the cell – discovering and defining, rediscovering, and redefining the structure and functions of organelles” (2000, p. 795).

Although it is beyond the time focus of this book, a survey of the membership of the ASCB in 1976 provides a good sense of how the organization grew during its first fifteen years. The survey was sent to all 2959 members of the ASCB in 1975; 1584 useable responses were returned. The membership was overwhelmingly white and male, although 23% were women. Over half had received their Ph.D. in the last ten years, and 69% in the last fifteen years. Universities and medical schools employed 75% of the members – 65% of those mainly on soft money.

### 3. CONCLUSION

The two brief historical sketches in this chapter are intended to illustrate the sort of decision making activities that go into establishing scientific institutions.<sup>26</sup> The founders of both the *Journal of Biophysical and Biochemical Cytology* and the American Society for Cell Biology were explicitly attempting to shape the discipline of cell biology. In both cases, the founders worried about being predominantly focused on morphology or tied to electron microscopy as a specific technique. Accordingly, they adopted a variety of

<sup>26</sup> I do not take this case to be unique. For example, in describing the founding of the American Physiological Society, Toby Appel (1987) showed how the founders set out to restrict membership to scientists publishing research in physiology, but also drew broad boundaries for the discipline of physiology.

strategies to increase participation by more functionally oriented investigators (for example, they invited such investigators to serve as editors, officers, or symposium speakers).

As it turned out, however, it was not possible to structure the journal or the society precisely as the organizers had envisioned. Including Schmitt, Bear, Davis, etc., on the board of editors and having them encourage submission of papers in biophysics failed to attract significant numbers of biophysically oriented papers to the *Journal of Biophysical and Biochemical Cytology*. Likewise, including Green, Novikoff, Siekevitz, etc., as officers and program chairs of the ASCB failed to attract significant numbers of biochemists to join or attend meetings regularly. The existence of other institutions for biochemistry and biophysics constrained the efforts of cell biologists to position themselves in the ways they sought. Yet, in another respect, those shaping the new institutions were successful. They did avoid the narrowness of becoming a journal or society solely concerned with morphological structure, and they did succeed in establishing a new scientific domain in which the structure and function of cell constituents were well-integrated. The point I want to emphasize is that this did not just happen; it was the product of conscious attention and strategic efforts by those scientists who participated in creating these institutions.